are described as if they dropped down ready made from the sky into their appropriate place.

Many superior designs could at this rate be made for the Forth Bridge; but then this ignores an important controlling element, that the bridge was to stand, not only when completed, but at every intermediate stage of the erection.

Even the operation of hoisting or rolling into place a forty-foot girder is not a simple matter; during the process the ordinary stresses are mostly reversed, and the structure runs the risk of "cockling."

We find no mention of the Tower Bridge, the most important experiment of a drawbridge *à bascule*. G.

The Amphioxus and its Development. By Dr. B. Hatschek. Translated and edited by J. Tuckey. (London: Swan Sonnenschein and Co., 1893.)

This is a translation of Dr. Hatschek's well-known paper on the subject published twelve years ago. It will no doubt enable those who cannot read German to follow Dr. Hatschek's statements. But unless the rest of the translation is more accurate than that of the title, readers will be deceived and disappointed. This book is not correctly called "Amphioxus and its Development." That is a salesman's title. There is nothing in it about Amphioxus, except an account of the earlier part of the development. The important facts of the larval development discovered by Willey, as well as the adult structure, are not dealt with. The original plates have not been reproduced in this translation, but very small and often obscure reductions of them are substituted.

E. R. L.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

The Use of Scientific Terms.

I am glad that so distinguished a physicist as Dr. Lodge has found certain matters relating to the history of physiology, which I discussed, I fear very imperfectly, in my Nottingham address, to be of sufficient interest to induce him to read and criticise it. Fully appreciating the geniality with which his criticisms are expressed, I will ask your permission to comment on one or two points in his letter, which may not be uninteresting to the readers of Nature.

One of the principal objects which I had in view in my address was to promote that intercommunication between the physical and physiological sciences which Dr. Lodge thinks so desirable, and I am no less sensible than he is that this solidarity is much impeded by inconsistency in the employment of words. Your correspondent avers that whereas the language of Physics consists in "simple English phrases" and "common words made definite by connotation," our biological words are "polysyllabic," and our modes of expression as unlike those of daily life as can be contrived. We say "devitalising," for instance, when we mean killing, just as the chemist says "desiccating" when he means drying.

It is difficult to express the complicated relations which exist between the phenomena of life without using terms which are themselves complicated. Thus, I venture, notwithstanding Dr. Lodge's good-natured pleasantry, to think that the word "chemiotaxis," bad as it may be, serves better to express the little that we know about the "particular go" of certain processes than any simple English phrase we could substitute for it.

Two words, "life" and "energy," are specially referred to by Dr. Lodge as examples in illustration of the inconveniences which are to apt to arise from their improper use. In Physiology the word "life" is understood to mean the chemical and physical activities of the parts of which the organism consists together with their co-ordination—not the processes only, nor their co-ordination only, but both at the same time. Dr. Lodge uses the word life without making it "definite in connotation," but from what is said about it, it is evident that the life which he has in view is not made up of processes, but merely consists in their co-ordination or adaptation for the purposes of the organism; for it is defined as the "power of *directing* (the italics are mine) energy into otherwise unoccupied channels." This being understood, all that Dr. Lodge says about life, and particularly his statement that it is *not* a form of energy, seems to me to be in accordance with the views that I endeavoured to set forth in my address. The only difference, therefore, that exists between us relates to the sense in which the word life is to be used for scientific purposes. Next follow some trenchant observations as to the misuse of the word "energy." I do not think that I am accused of such misuse. Nevertheless it may be useful to note that in referring to the sense in which J. Müller and his illustrious pupil had used the term "specific energy," it was expressly stated that their use of it was in a sense entirely different from that in which it is employed in physical science; and further, that the words quoted from the "Physiological Optics," viz. "energies of the nerves of special sense," were written in 1886, not "long ago," as Dr. Lodge suggests.

I can assure your readers that to the best of my knowledge the word "energy" is never used in the old sense by physiological writers, excepting, so to speak, between inverted commas; and with reference to the historical importance of Müller's doctrine, and still more of Helmholtz's earlier physiological writings, the words "normal activity," or others of similar import, are substituted for "specific energy," not as necessarily meaning anything quantitative, but simply the mode in which the organ normally reacts.

To the suggestion that "subjective light" should in future be designated by an impressive-looking word beginning with *photo* and ending with *taxis*, I have no objection to make, excepting that it might turn out to be rather sesquipedalian. May I add, that I hope to have the opportunity of recurring to the subject of the vision of the totally colour-blind.

J. Burdon Sanderson.

The Thieving of Antiquities.

A recent case, which has occupied some space in Nature, raises much larger issues than the character of individuals, and issues which must be faced sooner or later.

The present conditions of the laws and practice regarding antiquities is most unhappy, both in the interests of science and in the interests of museums. Two matters require much revision: (1) The modes of excavating; (2) the laws regarding excavation and exportation.

As to the mode of excavating it is still generally the custom to leave much in the hands of native overseers, and often the European in charge does not live on the work. Until it is recognised that it is unjustifiable to disturb antiquities without recording everything that can be observed, we shall remain in the state of mere plunderers, without a claim much higher than that of the treasure-hunting natives. In Egypt, hitherto, nearly all official excavations have been made by trusting entirely to uneducated and dishonest native overseers; and while the laws are strict concerning Europeans working, the natives plunder almost at their will under one pretext or another. With suitable regulation it has been proved practicable to entirely excavate a site without any loss or pilfering of the smallest objects by the natives; and such excavation, entirely under trained and educated observers, either native or foreign, should be the aim in all future work.

But in the matter of the legal position it is far more difficult to reach a satisfactory basis. Baldly stated the case stands thus. Every country in which there is anything much worth having, stringently prohibits exportation and excavation; and nearly all the growth of museums of foreign antiquities is in direct defiance of the laws. Most countries are engaged in thieving from others on a grand scale, by various underhand agencies; a form of thieving which is as much tolerated by public opinion as smuggling was in former days According to law, no antiquities of any kind can possibly leave Turkish or Greek territory, and nothing that is of great importance can leave Italian or Egyptian territory. Yet museums grow.

The actual course of affairs is that some private agent, or museum official, hears of something important, and buys it up

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in order to smuggle it for the museum in which he is interested. Sometimes museum officials go on missions to collect, or to excavate in accordance with the laws, while what they obtain is smuggled out in defiance of law. This is going on yearly, and will go on till some better system is established. Meanwhile all information concerning such discoveries has to be suppressed; and the most important acquisitions of museums are a matter which cannot be published, or even talked about in detail, while official papers have to be treated as secret archives.

In England the Government is a hindrance rather than a help to a better state of things. France and Germany ask other powers in a straightforward way for presents of antiquities by diplomatic channels; and they often get what they want, as we did in the days of Lord Stratford de Redcliffe and Sir Henry Layard. But recently English diplomacy has, on the contrary, repeatedly thrown away what rights Englishmen might claim concerning antiquities, in order to gain petty advantages which diplomatists were capable of understanding.

The work which has been done in Egypt by the Exploration Fund and myself, at least shows that such an unsatisfactory state of things is not unavoidable. The Egyptian laws are administered with more sense than such laws in other lands, and with a little diplomatic protection the position would be all that could be reasonably wished. For many years large excavations have been made openly, and with complete freedom, by Englishmen; nothing has been lost, either of objects or information, owing to surreptitious methods; all that has not been most essential for the country itself has been openly brought to assist study in England, and the fullest statements can be openly and honourably made on the subject. Meanwhile objects smuggled by officials have to be kept quiet, and lose whatever scientific value their record might have possessed.

Until our Government sees its interests in backing up work for its museums by honest methods, and straightforward dealing, we shall continue to lose the greater part of the scientific value of museum acquisitions, and to have a seamy side to our administration which is more discreditable than those personal questions that have lately been raised.

W. M. FLINDERS PETRIE.

University College, London, October 10.

The Glaciation of Brazil.

DR. WALLACE'S pointed reference to myself in this week's NATURE induces me to send you these few lines.

It has been said by more than one critic of my book on the "Glacial Nightmare" that in some cases I was merely slaying the slain, and notably in regard to Agassiz's views about the glaciation of Brazil. It has been overlooked that Agassiz's experience and authority on glacial matters were unrivalled, and that he had written on this very question: "An old hunter does not take the track of a fox for that of a wolf. I am an old hunter of glacial tracks, and I know the footprint whenever I find it."

Again, Dr. Wallace, whose knowledge of the tropics is so profound, had written: "Professor Agassiz was thought to be glacier-mad, but if we separate his theories from his facts, and if we carefully consider the additional facts and arguments adduced by Prof. Hartt, we shall be bound to conclude that however startling, the theory of the glaciation of Brazil is supported by a mass of evidence which no unprejudiced man of science will ignore merely because it runs counter to all his preconceived opinions." Again he says: "It can hardly be maintained that the discoverer of glacial phenomena in our own country, and who has since lived in such a preeminently glaciated district as the Northern United States, is not a competent observer; and if the whole series of phenomena here alluded to have been produced without the aid of ice we must lose all confidence in the method of reasoning from similar effects to similar causes, which is the very foundation of modern geology."

Lastly, Mr. James Geikie, in his second and revised edition of "The Great Ice Age," quotes Agassiz's conclusions without a word of protest or warning (*op. cit.* 484–5).

With these strongly expressed views before me, it was impossible to ignore the issue, and it can hardly be said I was slaying the slain in criticising those who believed in tropical glaciation.

I did not then know that in his subsequent work on Darwinism Dr. Wallace had, with that candour which makes his works so valuable to some of us, qualified and partially withdrawn his previous conclusions on the subject, a fact which he again emphasises in his letter to you. With this letter *cadet quæstio*, I know no one now who is willing to support Agassiz's theory, and we may take it to be dead. *Requiescat in pace.*

Meanwhile, however, let us do justice to those whose observations and logic have dispelled one phase at least of the glacial nightmare. Dr. Wallace attributes this to his friend and correspondent, but the work had already been done, and amply done, by others, as I tried to show in my recent book. In it I have quoted largely from the admirable remarks of Prof. Orton, Dr. Ricketts, M. Crevaux, and last, but not least, Prof. Hartt himself, who as far back as 1871 had given up Agassiz's views in regard to the Amazonian glacier (see *American Journal of Science*, 3rd ser. vol. i. pp. 294–5).

When we have got rid, however, of Agassiz and his Amazonian glacier, we have not got rid of all our difficulties. While we cannot accept the notion of tropical ice-sheets, we have still to explain the existence of erratic phenomena in the tropics, such as those described by Schomberg in Guiana, by De la Beche in Jamaica, by Blandford in Southern Persia, by Chardin in Media, by Belt in Nicaragua, and by Hartung in the Azores. There seems some difficulty in explaining these phenomena without invoking the former existence of local glaciers in parts of the tropics where they no longer exist, and also the occurrence of large diluvial movements there. I should be greatly indebted to Dr. Wallace, and so would others, for his views on this subject. There remains another and a more critical difficulty which I must reserve for another letter. In conclusion he will permit me to thank him for his very valuable and courteous letter.

HENRY H. HOWORTH.

30 Collingham Place, Cromwell Road, S.W.

The Glaciation of Brazil.—Scintillation of Stars.

A VERY cursory examination of the gneiss rocks about Rio de Janeiro—particularly the Corcovado—will show how the rock breaks up. In some places it comes off in great flakes like the coats of an onion, and the edges of these flakes are quite friable, and can be reduced to fine grains between the fingers. In many places it is found quite crumbled up by the weather, and down the coast towards Santos fine grains of these rocks can be found in the soundings at some distance from the land.

It is somewhat singular that observation has led me to a contrary opinion to M. Dufour in the scintillations of stars (NATURE, October 19). My attention was first drawn to the phenomenon by an old and experienced sailor, a native of the Western Islands, and a most clever weather prophet. I have constantly observed at sea that steadily-burning stars indicated calm, fair weather, and the more they twinkled the worse the weather was likely to be. The forecast given by this variation in scintillating was almost invariably correct in the high latitudes, though it failed sometimes in the tropics.

DAVID WILSON BARKER.

The *Worcester*, Greenhithe.

The Summer of 1893.

IN his letter in NATURE of August 31, Mr. W. B. Crump explains how the weather of the year has influenced the times of the flowering of the Halifax flora; and it may be of some interest to offer a note on the blossoming of a few common plants, trees, and bushes around Worcester.

The cardamine blossomed on April 16, herb Robert on the 16th, the oak on May 5, the elderberry on the 10th, the purple orchis on the 13th, and bear's garlic on the 13th also.

In this part of England field blossoms form an important factor in cottage economy. The harvest of this flora begins in spring with the primrose, the violet, and the wild daffodil, the latter here called the Lent lily. This season the Lent lily blossomed in March, as did the primrose and violet. Of late years these flowers have acquired a commercial importance, and engage, especially the former, a multitude of pickers and packers, lending life and colour to lonely railway stations. During the season dealers station in suitable country habitats agents who collect the flowers gathered by the pickers, and in large hampers despatch them to destinations all over the kingdom. This year the daffodil yielded less abundantly than usual.

Next to these blossoms follows the cowslip crop. This, for the sake of the pips, which, at 1*s*. a peck, are in demand at the British wine makers, is collected largely by cotters' children.

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